

SCIENCE.

FRIDAY, SEPTEMBER 17, 1886.

COMMENT AND CRITICISM.

ALTHOUGH THE GENERAL PUBLIC has ceased to be interested in the subject of cholera, which two years ago was the engrossing topic of daily conversation, the medical profession has not ceased to investigate its methods of growth and propagation whenever the opportunity has offered. A most important contribution has been made by Macleod and Miller, as a result of their labors in Shanghai during the past year. They found Koch's comma bacillus in twenty-five out of twenty-seven cases, and satisfactorily account for its absence in these two cases. In cases of diarrhoea and dysentery the bacillus was absent. The germ was destroyed on drying, but when kept moist was capable of growth after four months. Experiments are now being made upon guinea-pigs with a view of producing the disease in them if possible. Emmerich and Buchner, who have been studying cholera in Sicily, are of the opinion that the cholera germ finds its way into the body by means of the inspired air. The opinion commonly held, and among those of this opinion is Koch himself, is that it is through the mouth and stomach that the germs find their entrance.

THE FAITH CURE has succeeded in drawing to its ranks many individuals who, afflicted with incurable or imaginary diseases, have consulted regular physicians without obtaining any thing more than temporary benefit. From time immemorial this class has been a large one, and as a consequence the weapon ointment of Hildanus, the tar-water of Bishop Berkeley, the metallic tractors of Perkins, and the magnetic belts of Wilson, have, each in its turn, had their devotees and enthusiastic advocates. Rev. Dr. Buckley, editor of the *Christian advocate*, in an article contributed to the *Century*, declares that the tendency of faith-healing is to produce an effeminate type of character which shrinks from pain, and concentrates itself upon self and its sensations. He thinks that it destroys the ascendancy of reason in the soul, and that it tends to mental derangement. It might also be added, that such a belief, if general-

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ly accepted, would at once put a stop to all study and scientific investigation, and relegate us to the dark ages. The law which is from time to time enforced against the medical pretender and charlatan should be invoked to aid in the expulsion from the state of all, by whatever name they are known, who for mercenary purposes traffic in the innocence and simple-mindedness of the weak and the sick.

THE HIGHER EDUCATION of women was discussed by Dr. Withers Moore in his recent address to the British medical association. Inasmuch as his hearers were physicians, Dr. Moore wasted no time with an introduction, but at once went to the root of the matter by stating that the proper function of woman was to act as a producer of men, for she is the only means by which man can be brought into the world. The functions of gestation and maternity require a great outlay of physiological force, and, if this force is used up in other work, the offspring of the world must suffer, as must also the woman herself. There are two channels of expenditure of physiological force of the woman, — the terrible strain of higher and professional education, the training for competition with men in the most severe exercises of the intellect; and the expense of being properly trained for motherhood.

The saying, 'Educate a woman and you educate a race,' is full of promise if rightly interpreted; full of dire disaster, if it be applied to the mind to the exclusion of the body. Excessive mental labor is a cause of sterility; and whatever does, or tends to, render women infertile is prejudicial to a nation. Young women at the present time think that they must do men's work, though there is no necessity for it, and when they have not the constitution to do a woman's proper work. If girls were more properly taught at school the true physiology of woman, this would be in a great measure stopped. We cannot turn man into a woman, nor fit him to perform a woman's duties; no more can we fit woman to perform the work and duties of a man. Dr. Moore's address deals with this all-important question from an exceedingly practical stand-point, and is singularly free

from abstractions and generalities; and, if his views are to be controverted, they must be met with correspondingly practical objections. His treatment of the subject shows a large experience with the every-day life of the women of the present time, and will well repay most thorough and careful perusal.

WRITER'S CRAMP IS AN AFFECTION which, until a very recent date, has been looked upon as in most cases incurable. Fortunately, however, for those who suffer from this disease, means are now known to exist not only for its amelioration, but for its permanent cure. The difficulty is one which is not, as its name implies, confined to writers. It may occur in any individual whose occupation brings into constant play one set of muscles: thus the pianist, the telegrapher, and the ballet-dancer may suffer from these cramps or from an inability to perform the acts peculiar to his occupation. The cramps are merely symptoms of a diseased condition, the exact seat of which is a matter of dispute; some locating it in the brain, others in the spinal cord, while there are those who regard the nerve-centres as in no wise affected, but trace the source of the affection to the nerves themselves. The method of treatment which has been found most successful consists in the application of gymnastics, combined with massage, to the affected muscles. The rubbing, and sometimes a gentle striking of the muscles with a wooden bar, together with regular movements of the fingers or other defective part, are continued for several weeks, during which time not more than one hour daily is devoted to these exercises. During five years, Wolff, who has given special attention to this affection, has treated 277 patients. Of this number, 245 were writers; 32 were pianists, violinists, telegraphers, and painters. 157 were cured, 22 improved, and 98 not cured.

DR. S. W. ABBOTT, of the state board of health of Massachusetts, in the *Boston Medical and surgical journal*, Aug. 12, describes the method employed by Professor Walpert in testing the air of inhabited apartments to ascertain the amount of carbonic acid present. The air-tester of Walpert consists of a simple rubber bulb, of known capacity, connected with a glass tube, which is constricted at its further end. The bulb is filled with the air to be examined, and this air is then forced through a measured quantity of lime-water until the opacity produced by the formation of lime-

carbonate is so great as to obscure a black mark upon the bottom of the test-tube containing the lime-water. With very foul air, the bulb having a capacity of twenty-eight cubic centimetres, and the quantity of lime-water being three cubic centimetres, the mark is obscured after filling the bulb ten or fifteen times; while, if the air is as pure as it should be, the lime-water will become turbid only after the bulb has been filled thirty or forty times. Professor Walpert has prepared a table which indicates approximately the amount of carbonic acid present when the bulb has been filled from one to sixty times.

The principle upon which this tester is based is, of course, not new. It is, however, a much more convenient method than that recommended by Angus Smith, in which bottles containing lime-water were employed. All these methods are defective, necessarily so perhaps, for the reason that they all take it for granted that the amount of carbonic acid is a true exponent of the degree of purity of the air. This is, of course, erroneous. An air containing no more carbonic acid than that of the Alps, may, on account of organic impurities, be much more deleterious than one holding a large amount of carbonic acid, but without the organic contamination. There is much reason to hope that biological methods, with plate and other cultures, will help to solve this difficult question of practically ascertaining whether a given atmosphere is or is not contaminated to such a degree as to be prejudicial to health, and in what the danger consists. While the chemists and biologists are at work upon this problem, we shall still be confined to the estimation of carbonic acid present in the air, as an indication of its purity, and are glad to learn that Dr. Abbott has found Walpert's air-tester convenient in form and size, portable, and sufficiently accurate to meet the wants of the sanitarian.

THE CHARLESTON EARTHQUAKE: SOME FURTHER OBSERVATIONS.

FURTHER and more reliable observations and reports seem to confirm the substantial accuracy of the coseismal lines given in the map issued with the last number of *Science* to an even greater extent than could have been reasonably expected. The most disturbing element in compiling that map was the time of the earthquake at Charleston, as given by all the press reports and the signal-service observer at that city, 9.54 P.M. The most

accurate time found by Professor Mendenhall during his visit, however, was 9.51, which agrees very much better with all other accurate observations from adjoining localities. This and other reports might allow of shifting the central ellipse a little farther south; and this, of course, agrees better with the fact that the greatest destruction was caused at Charleston. Still, all reports agree that the shock was more nearly vertical at points to the north-west of that city, and even at Augusta, nearly due west.

Among all the recorded times, up to date, the following seem most worthy of mention at present: Charleston, 9.51, from very reliable data obtained by Professor Mendenhall; Washington, D.C., 9.53.30, the mean of observations by Professors Newcomb and McGee; Baltimore, 9.54, Richard Randolph, civil engineer; New York, 9.54, Western union telegraph operator; New Haven, Conn., 9.55.30, signal-service observer; Toronto, Ontario, 9.55, Professor Charles Carpmael, director of the Meteorological service of Canada; Mount Sterling, east central Kentucky, 9.53.15, I. J. Evans, watchmaker; Newport, Ky., 9.54.15, S. P. Newman, attorney; Portsmouth, southern Ohio, 9.55.57, signal-service observer, corrected by telegram to Columbus; Dubuque, Io., 9.58, as given in associated press despatches; Jacksonville, Fla., 9.54, associated press. If we take the centre of disturbance at a point about 50 miles north-west of Charleston, at 9.51 P.M., these figures give velocities about as follows: to Washington, D.C., 144 miles a minute; to Baltimore, 130; to Newport, Ky., 92; to Dubuque, Io., 103; to Jacksonville, Fla., 63. These seem to indicate that the origin was along a north and south line rather than at a point. If we take the origin at about the middle of the line joining Charleston and Raleigh, at 9.50 P.M., we get velocities as follows: to Washington, 76; to Dubuque, Io., 87; to Newport, Ky., 84; to Jacksonville, Fla., 71.

The area as stated in the last number of this paper is also about the same according to the latest observations. No reports of any disturbance are yet at hand from any point in southern Florida south of Tampa. It was felt very slightly at Boston, Mass., in northern Vermont, and New York; in Ontario it was very perceptible at Toronto and a number of points; and it extended into southern Michigan, eastern Iowa, Missouri, Arkansas, and Louisiana.

It is reported by Captain Boutelle that the water on the bar at Charleston has deepened since the shock from six inches to a foot. The character of the shock is reported from almost all points as decidedly undulatory, rather than vibratory, in character, which perhaps explains the fact that

there was generally formed a very fair idea as to the direction of the motion. Published requests for information have been very generously responded to; and although some letters are amusing, and even absurd, a large proportion of them give valuable and interesting data. The following extract from a letter received from Mr. Richard Randolph, civil engineer, Baltimore, Md., is such an excellent example of clearness of statement combined with accuracy of observation, that its perusal cannot fail to be both interesting and instructive:—

"I was reading in my front room, third story, east side of the street, about the middle of the block; was sitting with one leg thrown over and resting upon the knee of the other, so that the position of my body was nearly north and south, pointing with the foot about 15° west of north. While in this position, so sensitive to lateral oscillations, I experienced a sensation which I at first ascribed to a violent palpitation of the heart; but the absence of all uncomfortable feeling, and the great amplitude of the oscillations, quickly drove that idea from my mind. At the same time I was satisfied that such a motion could not be due to what I supposed to be a passing baggage-wagon loaded with trunks, the sound being exactly that of such a wagon, which frequently, during the last month, has passed over the cobble-stone pavement of the street. I did not look to see if there really was a wagon passing, and, although the sound began and ended with my observation of the telluric movement, I still assume it to have been caused by a wagon. After noticing for a few seconds my suspended foot swinging at right angles with the position of my body with the regularity of a pendulum, and feeling a general movement in the same direction, and hearing a sonorous beating of some object in my bedroom adjoining keeping time with these oscillations, I arose and walked across the room to my watch, and, upon inspection, saw that the minute-hand was exactly halfway between 9.53 and 9.54, i.e., 9.53½. My watch has, for the last two months, coincided precisely with the chronometers exposed for public reference in the windows of the principal dealers; and I had made a comparison only the day before. These chronometers keep the standard time of the Philadelphia meridian.

"While in this standing position, I no longer felt the vibration; but the sounds in the adjoining room continued at the same rate, but ceased, as did all perceptible vibration, by the time I resumed my seat, when I recorded on the blank leaf of a book I was reading, '6½ minutes of 10 P.M.'

"In order to form an estimate of the duration of the phenomenon, I held my watch before me,

and noticed the time required to repeat from memory the observations I had just made; and this indicated 45 seconds, and 10 seconds from the first sensation to the time of observation. In the same way I counted the number of beats per minute of the sounding body in the adjoining room, which indicated 110. As I have an ear for music and time, I have much confidence in this method of estimating.

"I then went into the other room to examine the object which caused the sounds, and found, that upon oscillating my wardrobe, which was backed against the north-and-south partition wall of the room, the sounds were produced by one of the doors tapping the partition between the two compartments of the wardrobe, giving out a not unmusical sound, and one that could not be evoked from any other object, and could only be produced by an east-and-west oscillation. To reproduce them with the intensity and periods during the earth movement required a movement of $\frac{1}{2}$ an inch at $6\frac{1}{2}$ feet from the floor, for a complete oscillation."

If many observers had the self-possession and skill to make such reports, the results would be valuable indeed.

EVERETT HAYDEN.

CHEVREUL'S CENTENNIAL FESTIVAL.

DURING the last two days, Paris has celebrated with unprecedented demonstrations of joy and respectful sympathy the centennial anniversary of the venerable *savant* Chevreul, unprecedented and unrivalled, because he is the only great scientist of our times who has attained the late hour of life he has entered this morning, Aug. 31, and because his life has been one of labor and hard work from the beginning. Chevreul's life is easily and shortly written. Like happy men and happy nations, he has no history, no adventures, no romance of any sort, but a simple, honest, straightforward, and manly life, given entirely to work, — to serious work, seriously conducted.

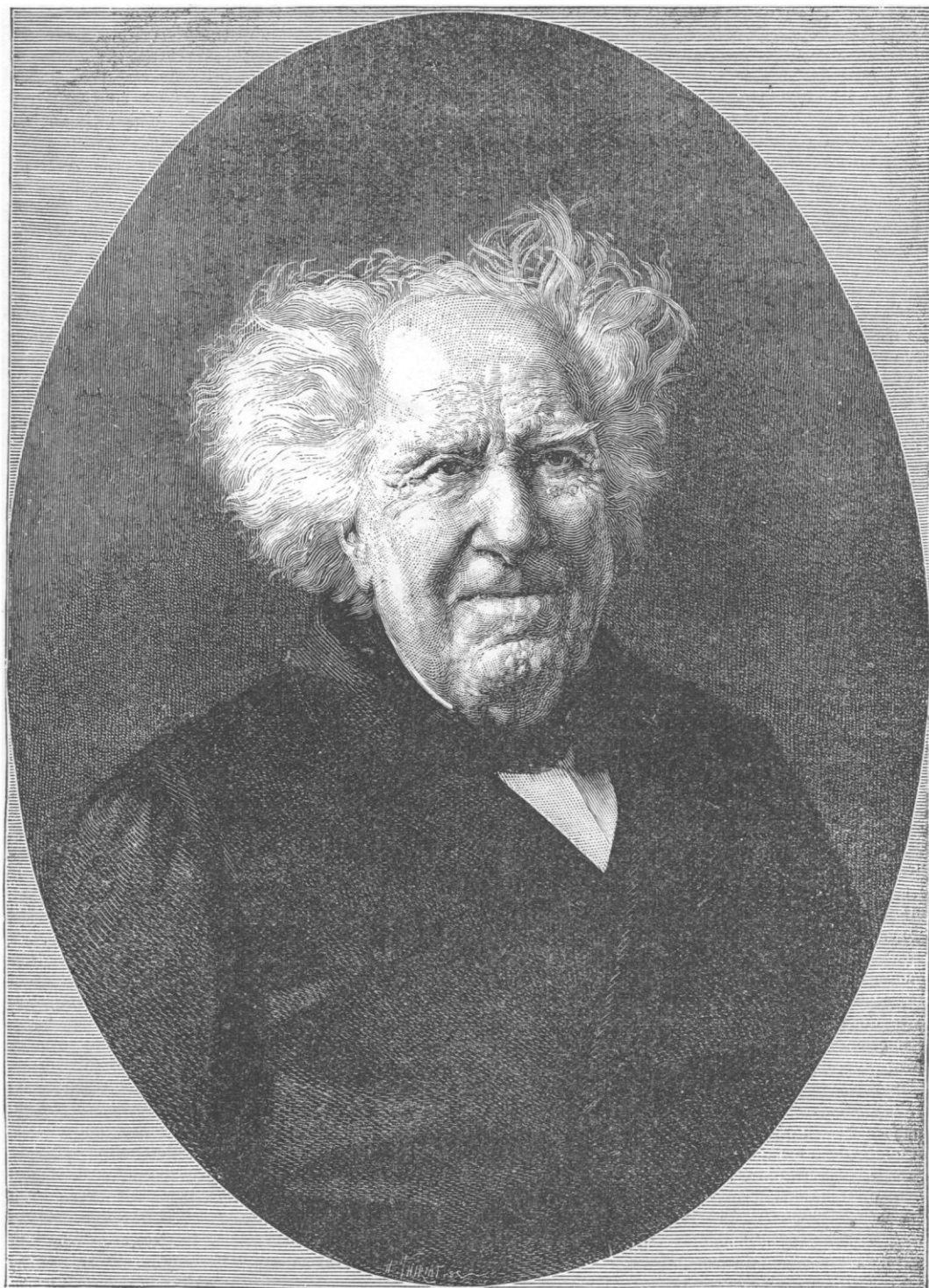
Michel Eugène Chevreul was born Aug. 31, 1786, in Angers. His father was a well-to-do physician in Angers, professor in the medical faculty, and a talented writer. Old age seems to be hereditary in the family; Chevreul's father having died at ninety-one, and his mother at ninety-three years. Chevreul is yet a very tall man, square in the shoulders, and walks quite erect and straight.

After the revolution the University of Angers was disestablished, a school for chemical and physical studies being put in its place; which school Chevreul attended between the ages of eleven and seventeen. In 1803, Chevreul went to

Paris, after having been taught the elements of chemistry by a professor named Héron. Chemistry was taught in Paris at that time by men of great science, Vauquelin and Fourcroy. Thenard was assistant to the former. Chevreul entered Vauquelin's laboratory, and set to work immediately. He was there with Orfila, Payen, Bouchardat, and Frémy, of which only one survives, Frémy, the present director of the Museum of natural history, on whose arm Chevreul leaned to-day when coming to the festival given in his honor. Chevreul's aptitudes were quickly noticed. In 1806 he was appointed director of Vauquelin's laboratory, and professor in the Lycée Charlemagne, and during the same year he published the results of his first experiments. In 1806 seven papers came from his pen, of which three were on coloring-matters (indigo and Brazilian wood). Four years later he was appointed *aide-naturaliste* in the Museum of natural history, then examiner for the Ecole polytechnique; and at thirty he was professor of chemistry in the Gobelins, the world-known manufactory of tapestry, and director of the department of tinctorial baths. In 1826, after the death of Proust, Chevreul was appointed member of the Academy of sciences, to which he has belonged ever since. Not one of his colleagues of that time is yet living. In 1830 he became professor in the museum, and some time after director, holding the former position till the present day, though not so actively the last two years, and the latter till 1883. He is a member of a great number of foreign scientific societies, and since 1875 has attained the highest dignity in the order of the *Légion d'honneur*. He never misses a meeting of the Academy of sciences, and it is not long since one could meet him in the Rue des écoles, walking to the institute, hat in hand, and hands behind the back. He seems to have an aversion to hats, and dispenses with them a great deal.

During the war of 1870 he remained in Paris the whole time of the investment, and lived in the museum, notwithstanding eighty German bombs scattered to pieces the magnificent hothouses of the Jardin des plantes, and one fell quite close to his own laboratory. It was in a letter written during January, 1871, to Abbé Lamazon, in answer to a note of the latter, that Chevreul used for the first time the expression he prefers when speaking of himself, — 'the dean of French students.'

Chevreul married early, but his wife died more than twenty years ago. His conjugal life was a very quiet and happy one. Chevreul has only one son, who lives in Dijon, and is a retired magistrate. He himself lives alone in Paris, devoted to his books and laboratory, both of which



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take all his time. He is very fond of fine books and rare editions.

As a man, Chevreul has a very pleasant expression, and always greets strangers or friends in a very hearty fashion. There is nothing pedantic in the man: he is amiable, kind-hearted, and good-natured. He is also a very just man and a stanch friend, which is shown by an incident that occurred in 1874. At that time, M. de Cumont, one of the most incompetent and unpopular ministers of public instruction that we have had of late, had made up a list of persons upon whom he wished to confer the distinction of the *Légion d'honneur*; but these persons, belonging to the museum, Chevreul found, with good reason, rather unripe for the much coveted distinction, while persons who were quite worthy of it were not put down on the list. Minister and director disagreed, and the director sent in his resignation. This created quite a sensation, and the minister had to abandon his project to induce Chevreul to withdraw his decision.

Upon the whole, Chevreul's life has been a very quiet one, devoted wholly to work and study. He is a rich man, as he spends very little, and his income exceeds by a great deal his expenses. He is a society man, and has very refined and pleasant manners. A few years ago he sometimes went to balls, and was a favorite with many ladies, who had great pleasure in listening to his conversation. He has a humorous turn of mind. Recently, when accepting a new assistant, he exclaimed, "Well, you must be plucky to become my assistant: I have already killed four!" "Killed" is a metaphor, but no more so than it is when used in speaking of a commander who has killed two or three horses, that is, has had them killed under him.

Chevreul's material life is very simple: he eats little. Two eggs and a slice of patty are enough for the morning, with some milk and coffee; in the evening, a full plate of soup, a cutlet, and some fruit, some cheese, and only water or beer, no wine at all.

A catalogue of Chevreul's works would be a work in itself. The two most important branches of science studied and developed by Chevreul are the chemistry of fat substances, and the theory of complementary colors. By his researches in the former of these, Chevreul has given methods for obtaining a number of very important and useful substances, such as stearin, glycerine, etc. Millions have been earned by the application of his methods.

The centennial anniversary of Chevreul was celebrated by various ceremonies. The Société d'agriculture, of which he has been a member for more than fifty years, and president for thirty-

seven years, gave him, during the meeting of Aug. 30, a handsome present, the 'Penseur' of Chapu, a very fine bas-relief, and a medal. After the meeting of the Agricultural society came that of the Academy of sciences. Chevreul went, as usual, and was greeted with great sympathy by his colleagues. The same evening Chevreul was invited to the opera, where he had not been for a very long time; and he assisted at the whole play, gotten up in honor of his first century. He had received before, the visit of the Chinese ambassador, Tcheou-Meow-Ki, who came to congratulate him; he had received a delegation from the inhabitants of the Rue Chevreul, who sent him a fine nosegay; he had received also the visit of MM. Alcan and Ch. Richet, who brought him a volume especially published for the occasion, 'Hommage à Chevreul,' and written by Berthelot, Gautier, Grimaux, Ponchet, and others, — a sort of *livre jubilaire*, as is often published in Germany when some important date in the life of a professor or scientist is celebrated.

Notwithstanding all these speeches and ceremonies, Chevreul slept very well, and was quite fresh the next day, when took place the ceremony of the unveiling of Chevreul's statue in the new gallery of the museum, which was hung with Gobelin tapestries of splendid hues and dimensions. Although a great many persons of scientific pursuits are still out of Paris, the room was full, and the heat was awful. A large crowd gathered in the Jardin des plantes to witness the passage of those invited. When Chevreul came in, leaning on Professor Frémy's arm and on that of an old soldier ninety-four years of age, unanimous applause went up, with many cheers for the hearty old *savant*. Chevreul was quite astonished at the sensation he created, and murmured, 'What a fine crowning-point for a *savant's* life!' The statue was due to Guillaume, and is a very fine one. It does not resemble much the Chevreul of to-day, but is so well arranged, and with so much taste and art, that the effect is excellent. This will be one of Guillaume's masterpieces.

But now began the unpleasant part of the day. It was half-past two, and the heat was terrific. One required rain, or hail, or snow, any thing that could refresh the air: nothing poured but speeches and large drops of perspiration. After one speech, another: one man went down the tribune one side, another got up the other side. They were audible only in a very small part of the building. The gallery was intended to receive specimens of natural history, and the skeletons of all creatures that had one, and was very well designed; but it was not intended for speeches. The consequence was that very little of them was

heard. One of those which was received with most enthusiasm was that of Nadault de Buffon, a descendant of our great naturalist, an old man already, and quite blind, but possessed with a strong voice, a fine appearance, and very enthusiastic feelings.

After this avalanche of speeches, which was certainly enough to fatigue one, M. Chevreul answered some words in reply, and the delegations defiled before him. The number of them was very considerable, but he witnessed the whole proceeding nevertheless. All saluted him with utmost respect and demonstrations of great joy. He was presented with a nosegay—I know not by which delegation—that was a masterpiece of art in the choice and distribution of colors. No more delicate allusion could be made to the venerable master's theory of complementary colors; and it was understood by the whole crowd, being exemplified in an unparalleled manner.

The ceremony was over at four o'clock. There were crowds of people around the monument, awaiting the passage of the centenarian, and progress was difficult and trying. However, all went very well, and Chevreul was received by the crowd with deafening applause.

It is likely that our readers would have been tired after all this ordeal: Chevreul was not. The same evening he was present at a great banquet given in the Hotel de Ville, and he even drank some champagne when his health was proposed,—a somewhat superfluous motion, it seems. During the night a torchlight procession paraded the streets; but this popular demonstration had nothing interesting in it, and no *savants* were concerned in this masquerade, which certainly originated in the brains of some alderman desirous of more votes at the next election.

Upon the whole, Chevreul's centennial anniversary was celebrated as it ought to have been, and as becomes, at the same time, a man of high scientific standing, and a city which always appreciates great thoughts and a noble life.

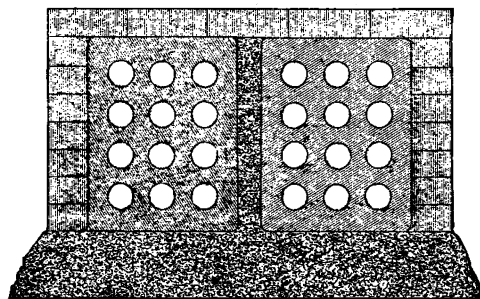
H. DE VARIGNY.

BURYING THE WIRES.

THE actual work of preparing subways or underground conduits to receive the telegraph and telephone wires, in this city, was begun on Aug. 30. Numerous experiments and tests were made, and many projected methods considered, before the commissioners having the matter in charge decided upon a plan which seemed to them satisfactory. The system finally selected appears to fulfil all the requirements of the case, and the work of 'burying the wires,' so long

discussed by the daily papers, will now proceed as rapidly as the conduits can be placed in position.

The subway, as now being constructed in Sixth Avenue, consists of a double row of conduit-blocks, laid in a trench five or six feet deep, with man-holes at every cross-street, for the insertion of wires and making connections. Each block is 42 inches long, 17½ inches deep, 13½ inches wide, weighs about 450 pounds, and is pierced by twelve 2½-inch holes. The material of which the blocks are made is a concrete composed of 80 per cent clean, sharp sand, 19½ per cent coal-tar pitch, and ½ per cent oil and black oxide of manganese. These are thoroughly worked together in a tank at a high temperature. The mixture is then forced into moulds of proper size and shape, subjected to heavy pressure, and deposited to cool in tanks of water. At a public test conducted by Mr. Albert R. Ledoux, chemical expert to the subway commission, the crushing resistance of this concrete was found to be 4,591 pounds to



the square inch, and the crushing resistance of a conduit section was 59,210 pounds. This concrete deteriorates and disintegrates in a few years where exposed to great changes of temperature, so that it is not adapted for use in pavements, or where exposed to the heat of the sun and the action of snow and frost; but the experience of several years proves that it undergoes no appreciable change when used under ground. As a material for sewer and drain pipe, etc., it has been found satisfactory.

The conduits are being laid in a manner that ought to insure their stability. At the bottom of the trench is laid a bed of cement concrete six inches in thickness. This is allowed to harden, after which the sections are placed in position, in two rows, leaving a space of two inches between the rows, to be afterwards filled with hydraulic cement. The method adopted to insure the continuity of the 'ducts,' or holes, through which the wires will run, is simple but effectual. The holes are moulded with a slight enlargement at the

ends, sufficient to admit a short tube or ferrule of the same inside diameter as the hole. As each block is lowered into the trench, and placed in position, a large plate of iron, previously heated, is held between one end of it and the end of the block it is to join. The heat softens the pitch, and removes any oil which may have been left by the mould. The iron is then removed, the block drawn back a few inches, and the ferrules are put in place. These ferrules are of such length, that, when pushed firmly into place against the shoulders of the enlargement, the blocks remain about an inch apart. The block, with ferrules inserted, being in position, a round wooden bar, split lengthwise into two long wedges, is inserted into each hole or duct, running back through the ferrules into the other block. One part of each bar is then slid upon the other, until they fill the hole snugly, the result being that the blocks are brought into practically exact alignment. Next, iron plates, embracing the joined ends of the blocks, are clamped in position, and the space between the blocks and surrounding the ferrules is filled with hot pitch concrete solidly rammed. Then the aligning bars are removed, and the operation is repeated with each subsequent block. The space between the conduits is filled with hydraulic cement, and the double conduit enclosed in brick-work, the completed subway presenting the appearance shown in section in the accompanying diagram.

THE STANDARD TYPOGRAPH.

THERE is now being perfected in this city a machine intended to dispense with type and typesetters in certain kinds of printing. The 'standard typograph' is the name selected for it by its inventors, though the term 'matrix puncher' would be a more fitting title. A good idea of its general appearance may be gathered from the accompanying illustration. At first glance, it seems to be a combination of an enlarged type-writer and a sewing-machine, possessing the key-board of the former and the stand and operating mechanism of the latter. The typograph is in reality a kind of type-writer, but, instead of printing upon paper, it produces indented or depressed characters upon a sheet of soft metal, from which an electrotype may be made, as from the wax matrix taken from type, in the usual electrotyping process.

The principal parts of the machine are, the key-board, resembling that of the Remington type-writer; the type-wheel, which revolves in a horizontal plane; and the matrix carriage, immediately above the type-wheel. Part of the last is shown in the engraving, above the key-board,

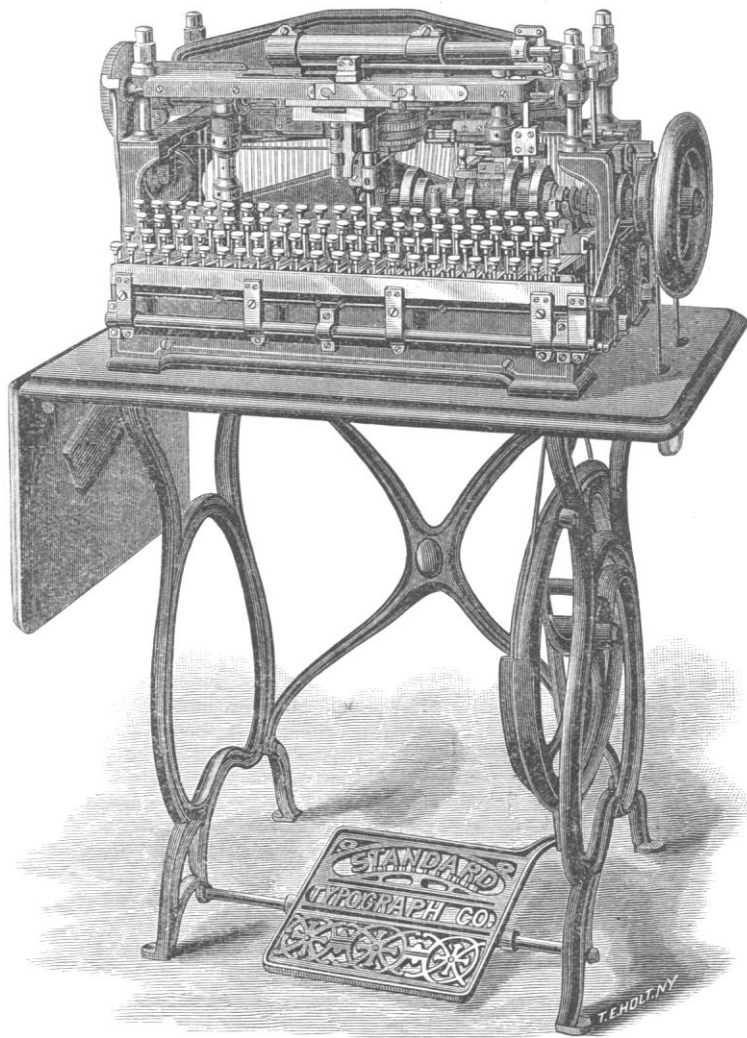
about the middle of the machine. Fitted in vertical grooves in the periphery of the type-wheel are a number of steel types, one for each character used in ordinary printing, the face of the type being upward, toward the matrix carriage. Two small lugs or stops project from the wheel at diametrically opposite points. Arranged in a semicircle at the rear of the wheel are two rows of detent levers, the outer end of each lever being connected by a link with a finger-bar of the key-board, much as the type-bar of a type-writer is connected with its key. The detent levers are pivoted near the inner end, so that the depression of a finger-bar, or key, as it may be called, raises the inner end of its connected lever into the plane of revolution of one of the stops of the type-wheel, each stop being located on the wheel slightly above its corresponding semicircle of levers.

The matrix carriage, one end of which is shown in the engraving, above and to the left of the type-wheel, has movement in two directions in a horizontal plane. The side movement, from left to right or *vice versa*, is communicated to the carriage by the return of a key to its normal position after being depressed to form a character in the matrix. This side movement, or letter-spacing, is variable, and is governed by the key depressed, so that the carriage is moved each time a space equal to the exact width of face of the type impressed in the matrix. Thus, for the letter *h* or *g*, the carriage would move twice as far as for *i* or *l*. By a simple adjusting device, this movement may be changed so as to leave a space between the letters, as shown in the concluding line of the sample paragraph given farther along. The other movement of the carriage, that required to bring the matrix into position for a new line, is produced by depressing a key provided for that purpose. This movement also may be varied so as to leave greater or less space between the lines.

The manner of operating the machine is as follows: the matrix, which, as at present used, is a sheet of lead about one thirty-second of an inch thick, is secured firmly to the carriage, and adjusted, face downward, in its place above the type-wheel. The operator, having his 'copy' within easy reading distance, puts the type-wheel in motion by means of the treadle, and depresses the keys one after another, according to the word or space desired, as in the ordinary type-writing machine. As each key is struck, the end of its detent lever, by contact with the projection on the wheel, stops the revolution of the latter, holding it in such a position that the type desired is in place for striking the matrix at the proper point. At the same instant the type is forced upward by a revolving cam, producing an impression of its face

in the matrix. The cam continues to revolve, the type drops back into place, the wheel is released and continues its rapid motion, and the carriage moves into position for the next letter, the whole operation being practically instantaneous, and the rapidity of working being limited only by the skill

and the last letter is struck. The 'feed' of the carriage is then reversed, and the rest of the line filled in backward. This is done so that the ends of the lines may be even, as the spacing between the words is not automatic, but depends altogether upon the skill and accuracy of eye of the



THE STANDARD TYPOGRAPH.

of the operator and the speed of the treadle. The next letter is produced in the same way ; and so on to the end of a word. Then the carriage is moved ahead sufficiently for the space desired by touching an appropriate space-key. This is continued until within two or three words of the end of the line, when the carriage is run on to the end,

operator. One line finished, the carriage is returned to the starting-point, the line-spacing key is struck, bringing the matrix into position for the next line, and the various operations are repeated. When the matrix is completed, it is removed from the carriage, and an electrotype taken ; and this electrotype is supposed to take the place of

type on the printing-press. The following short paragraph is printed from an electrotpe made direct from a leaden matrix produced upon the typograph:—

Although more than four hundred years have elapsed since the art was invented, it is a singular fact that the bulk of the world's printing to-day is done with movable types "set up" on the same plan adopted originally.

To the eye of the practical printer there are several defects in the above sample paragraph. The first word is not set in far enough from the end of the line. That is the fault of the operator, for which the machine cannot be held responsible. Some of the letters are too close together, and others too far apart; the letters do not range well, giving an irregular or 'squabbed' appearance to the line; some of the letters, notably the *e*, do not correspond in size with the others; and some, again, appear to be higher than the rest, giving a blacker impression. All these defects are doubtless due to imperfect workmanship on the part of machinists and type-cutters, and may easily be overcome in a more perfect machine. Another defect noticeable in the work of this machine is uneven spacing between the words. In the machine as at present constructed, this defect cannot be remedied except by almost superhuman skill on the part of the operator; but the inventors claim that in future machines, and with type cut on a 'unit' system (the width of face of each type being a known multiple of some unit taken as a standard), justification will be as easily accomplished as in ordinary type-setting. This remains to be seen. Still another defect, and a very grave one, is the difficulty of correcting errors. A wrong letter in a word, or a wrong word in a sentence, if about the same size as the right one, may be corrected by smoothing down the metal and repunching over the smoothed surface. But the omission of one or more words, or their repetition, can be remedied only by a new matrix, whole or in part. Neither can changes be made in the wording of a sentence, something frequently desired by writers upon inspection of their proofs. With 'copy' prepared exactly as it should be printed, and an operator proof against error, this defect would not be conspicuous; but perfection is no more prevalent among writers and operators than among inventors and machinists.

But were the machine perfect in all other respects, there is still one defect which practical printers who have examined the typograph and its work consider fatal, and one which, in the opinion of experts, will be found extremely difficult if not impossible to overcome. When the

steel type is forced into the soft metal of the matrix, it leaves a perfect impression; but, when the next type is forced in, the metal is pushed aside to a greater or less extent, thereby disturbing the previous impression, and preventing the full face of all the type from 'showing up' in print. This defect will be readily seen by comparing the sample paragraph with the contiguous paragraphs, which are printed from ordinary type. In the last three words of the sample paragraph, the metal has not been so much disturbed, as the letters are purposely set some distance apart, that there may be a protecting wall of metal left between them. Of course, it may be possible to discover some material for the matrix that will give better results in this respect, or some way may be devised to punch the metal without forcing it aside. These problems remain unsolved. In its present stage, the typograph shows an important advance in the direction of cheaper and more rapid book and newspaper work; but much yet remains to be done before the machine can be placed upon the market as a commercial and typographical success.

NOTES AND NEWS.

FROM a correspondent in Tokio we learn that on his return from America he presented a report on the resolutions of the Washington meridian and time congress. A committee was appointed by the proper authorities to discuss the matter, and on the 12th of July an imperial decree to the following purpose was issued: first, the meridian passing through Greenwich shall be the initial meridian for longitude; second, longitude shall be counted from this meridian in two directions up to 180°; third, the time of the meridian of 135° east shall be used as the standard time throughout Japan.

—A fireman on the steamer *Alvo*, which lately arrived in New York from Central America, was taken sick, and entered St. Vincent's hospital for treatment. The fever from which he suffered simulated yellow-fever to such a degree as to make his removal to the Reception hospital of the health department advisable. The attack proved fatal, and an autopsy revealed a yellow liver, a stomach filled with blood, and the other organs jaundiced. It was the unanimous opinion of the physicians present that yellow-fever was the cause of death. It is as yet unexplained how and where the disease was contracted, as it is reported that the health officer of the port never knew of yellow-fever existing at any port at which the *Alvo* had been.

—The report of F. H. Wines, special agent of the tenth census, on the defective, dependent, and

delinquent classes, is full of interesting details. The number of males confined in prisons and work-houses in the United States in 1880 was 53,604, and of females, 5,005. The number of prisoners to each million of the population was 1,069: in 1870 it was but 853. There were 1,833 insane persons, 1,533 idiots, and 976 blind persons to each million inhabitants. There were 21,595 out-door paupers, and 66,203 inmates of almshouses, during the census year.

—A writer in the August number of the *Nineteenth century* begins an interesting discussion on the question, 'Are animals happy?' The view that the author takes practically amounts to saying that an animal with a less highly organized brain is more apt to be happy. While many of his inferences are suggestive, they are not founded on that broad knowledge of biological facts which the subject deserves. The article has been severely criticised in English periodicals.

—It has recently come to light that the state of New York, in 1806, paid to John M. Crous a thousand dollars for a remedy against hydrophobia which he considered infallible. The measure was advocated by DeWitt Clinton and Chancellor Kent. This remedy consisted of one ounce of the jaw-bone of a dog, burned and pulverized; the false tongue of a newly foaled colt, dried and pulverized; and 'a scruple of verdigrease,' raised on the surface of old copper by laying it in moist earth. The warrant of the comptroller on which the money was paid, and the receipt of Crous, are on file with other state papers at Albany.

—The submarine torpedo-boat briefly described in *Science* recently is having some changes made in its machinery and in details of construction. It will be equipped with horizontal rudders at the bow, in addition to those at the stern, so that it may be submerged with an 'even keel.'

—One of the amateur aeronauts whose experiences are recorded in the September number of the *Century* makes the interesting observation, that, immediately upon alighting, all remembrance of the events of the journey is gone, and only after several hours can he recall his experiences. He adds, that after a battle, soldiers have experienced the same phenomena.

—The temperature of August, 1886, was over four degrees above normal in Iowa, being the hottest on record for over twenty-five years, excepting August, 1881, which was one degree warmer still. The number of hot days on which the temperature reached or exceeded 86° was twenty-one, which is the highest ever recorded, being three in excess of August, 1881.

LETTERS TO THE EDITOR.

***Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Barometer exposure.

MR. CLAYTON'S last letter on the above subject shows that he has been considering all along the so-called 'serration effect.' It seems to me that this narrows down the problem very materially. Kindly allow me space to emphasize a point or two that I laid down in my previous letter, and to give a few facts regarding the above effect. I still think, that, if the wind has a tendency to diminish the pressure in a room by blowing across a chimney communicating with it, there will be a slight draught up the chimney, of the air forced into the room on the windward side. Mr. Clayton's theory of a smoky chimney is satisfactory only as a theory, but the facts are all against him. Any builder will tell him that a properly constructed chimney will draw under the conditions mentioned by him; namely, 'wind blowing across it.' He will also find a large number of cases in Boston where chimneys have been built up in such a way as to make the effect suggested by him a maximum; and this, too, to obviate a lack of draught.

I have studied the above serration effect with some care, and have compared wind-velocity and barograph sheets for nearly two years. The barograph is enclosed in a rather tight case, and its sheet has a motion of about .75 of an inch per hour. The highest wind noted was 36 miles per hour, though there may have been momentary gusts reaching 50 miles per hour. The effect was noticed rather faint with a velocity of 4 miles per hour. Several times a velocity of 28 miles per hour produced no effect; and velocities of 20 miles per hour, with no effect, were quite frequent. Almost all the cases occurred with relatively low pressure, and they were often mixed up with the irregular fluctuations so often noticed at the centre of a cyclone. The maximum effect below the middle line was about .010 of an inch, with a mean value of about .005 of an inch. One singular fact noted was that often there would be a jump of .010 of an inch above the general trend of the line of pressure. A part of this might be caused by the momentum of the pencil, but this would be very slight. It would be very interesting to try the effect of opening a window to windward while the serration effect is taking place. I believe this has already been done to some extent, with the result that the serrations are not materially changed. At all events, I find that among those who have studied the problem in the light of these barograph sheets, there is a well-established belief, that while the wind has an influence in producing the serrations, yet just how it acts is problematical. Any such serration effect by the wind as .100 of an inch below the general pressure line is well-nigh incredible. I am so strongly convinced on this point, that I am perfectly willing, for the benefit of the readers of *Science*, to be at a little expense in order to enable us to see this effect for ourselves. If Mr. Clayton will make a tracing of one of his best effects, and send it to *Science*, I will furnish the editor with the funds necessary to reproduce it. I impose but two conditions, and will modify these if Mr. Clayton thinks them too severe. The conditions are, 1°, the chimneys or trap-doors of the house, or connecting with the room, where the barograph is, must be near the centre of a rather flat roof, that is, not at the edge or near the

ridge-pole of a rather steep roof; 2°, the tracing must exhibit the serration effect for at least one hour, and must have in that hour not less than two downward motions of the pencil at least .050 of an inch below the general trace of the pressure at the time.

GAN.

Sept. 10.

'Communistic leanings.'

In your reports of papers read at the Buffalo meeting of the American association for the advancement of science, you refer, in the following terms (*Science*, Sept. 3, p. 219), to a paper read by me before the section of political economy and statistics: "The theory of rent and its practical bearings was discussed by Edward T. Peters of Washington, and with such communistic leanings as to meet little approval."

This language is calculated to convey to the reader's mind an entirely erroneous idea of the paper referred to. That it is not based upon knowledge will appear from the fact that the title quoted is one which I submitted when my paper was only in part written, and for which I afterwards substituted a title better suited to the narrower ground to which, on the score of time, I found it necessary to confine myself. That title, as may be seen by referring to the programme of proceedings for Aug. 24, was 'Errors in the Ricardian theory of rent.' In the treatment of this subject I was not conscious of any 'leanings' except a leaning to scientific truth, my paper being simply an attempt to determine whether certain propositions embraced in the Ricardian doctrine logically flow from the assumptions upon which the doctrine is supposed to be founded, and also to compare them with certain very conspicuous economic phenomena, in order to ascertain how far the theory agrees with the facts of experience.

I will not ask space for a statement of my views on the general subject in question; but it would interest me to know whether *Science*, which may be supposed to appreciate the significance of words, and to use them responsibly, — which, moreover, has of late done itself honor by the breadth of its hospitality to various shades of economic thought, — would stigmatize as 'communistic' the proposal of John Stuart Mill "to intercept by taxation for the benefit of the state the unearned increase in the rent of land;" whether it would apply a like epithet to the proposal of Dr. Adolph Wagner, the distinguished professor of political economy in the University of Berlin, "that municipalities [I quote from 'Land and its rent,' by President Walker] should purchase all town property, in order to realize therefrom the progressive increase of values;" or, finally, whether the character of an opinion, and the epithets fitted to describe it, depend entirely on the degree of prominence of the person from whom it emanates.

I observe, in the first paragraph of your report of the proceedings of Section I, the statement that the section had, at the Buffalo meeting, "been comparatively free from the attacks of socialistic and economic cranks, to which it is especially subject." I trust it will always be successful in keeping off 'cranks' of every description; but I quite as earnestly hope that no sickly fear of giving audience to unpopular opinions will induce it to set up a narrow philistine standard of economic orthodoxy, and brand as 'communists' or 'cranks' all who fail to conform to it. The 'approval' of a body conducted upon such principles could be readily dispensed with.

Political economy, as Prof. H. C. Adams, in one of the excellent economic papers recently published in *Science*, has well said, might be appropriately defined as the science which 'treats of industrial society.' Its especial province is, therefore, in a large degree, the arena of clashing interests; and unless Section I of the American association proposes, as a section of 'economic science,' to enact the play of Hamlet with the part of Hamlet left out, it must always, from the very nature of its functions, be 'especially subject' to the introduction of disturbing social questions, and must often hear views advanced which, however sound in themselves, and however disinterestedly scientific in their origin and spirit, will meet but 'little approval' from the men or classes whose interests or prejudices they may happen to antagonize.

E. T. PETERS.

Washington, D.C., Sept. 9.

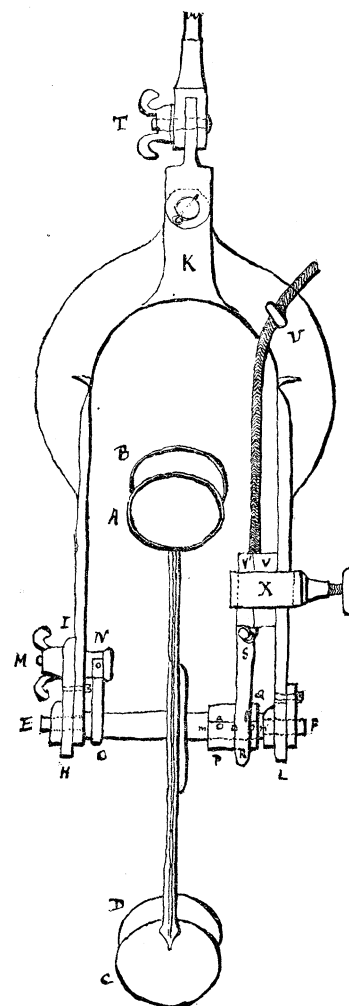
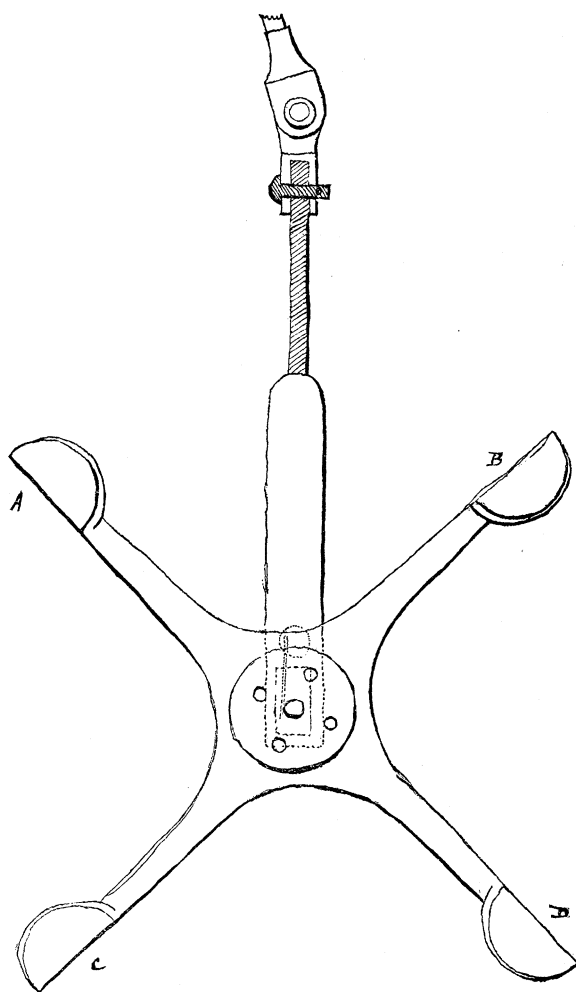
An electric log.

In May, 1882, I sailed from Marseilles for the Piræus on the steamship *Ava*; Capt. Aug. Bretel, of the *Compagnie des messageries maritimes*, commanding. A short time after going aboard, I noticed a small rope running through the saloon over the cabin doors to the after skylight, and thence along the side of the ship to the stern, where it was made fast. The next day I saw the captain and the first officer looking at a curious instrument, which looked something like an aerometer, except that the cups revolved in a vertical position. This instrument was fastened to the rope which I have mentioned, and thrown overboard, the captain meanwhile watching the revolutions of the wheel through a powerful field-glass. As it did not seem to work satisfactorily, it was hauled in; and I noticed that the captain, in making some repairs to the rope, used a stick of Chatterton's compound. This led me to believe that there was a copper conductor in it, and that electricity in some form was being employed. There was no opportunity at that time to make inquiries; but a few days later the captain kindly permitted me to see the instrument, which he called a 'loch-moulinet,' or 'electrical-mill-log.' After throwing it again into the water, he took me forward and showed me the earth connection, which was soldered fast to one of the iron beams of the ship. Thence the wire went through the chart-room to the wheel-house, where there was a telephone. This electric log, it seems, was the joint invention of Capt. G. Fleuriat and Bretel, and was so arranged, that, when connected with the cable, it formed part of an electric circuit, which was opened and closed with every revolution of the copper shaft to which the four cups or hemispheres were attached. The number of revolutions made by the shaft in a given time was of course dependent upon the speed with which the cups were dragged through the water; in other words, regulated by the rate of motion at which the ship was moving. A table had been prepared by the inventors, showing the number of knots per hour corresponding to the number of revolutions of the shaft in a half-minute. On placing the log and telephone, so arranged that it could be switched, in circuit, every revolution of the shaft, and consequent closing of the circuit, caused a click, plainly audible to any one listening at the telephone. The log having been allowed to run out to such a distance as to be practically free from the influence of the screw, I listened

at the telephone. The sound, like a tapping on the diaphragm of the telephone, came clear and distinct; and, when the captain turned the half-minute glass, I had no difficulty in counting the revolutions of the shaft, seventy-four in number. On referring to the table, it appeared that we were making eight and five-tenths knots, which was the exact speed of the ship as shown also by the revolutions of the engine. The experiment was very satisfactory and extremely interesting. Captain Bretel wrote out a description

ing through the shaft, which screws make electrical connection between the copper strip and the shaft. An elastic tongue, *O*, on the screw *N*, bears upon the other end of the shaft, giving metallic contact between shaft and jaw. The log is towed some distance astern of the vessel by a log-rope, which is attached to the jaw by a copper shank and a clamping-screw, *T*. To this copper shank is secured a strip of zinc, thus forming a weak galvanic battery.

An insulated wire furnishes electrical connection



of his invention, with drawings, which he gave to me.

In the accompanying diagram, *A, B, C, D*, are four hemispherical cups on the ends of four arms affixed to a brass shaft, *E F*, which revolves freely in lignum-vitae bearings at the extremities of the jaw *H K L*. For convenience in removing the shaft, the bearing at *H* is detachable, being secured in place by the wing-nut *I*. At one end of the shaft is a small lignum-vitae pulley, *P Q*, having on one side a strip of copper, *mm'*, fastened in position by screws pass-

between the commutator brush or tongue, *R S*, and a Bell telephone in the chart-room of the ship, the wire making several spiral turns around the log-rope to guard against snarling. With the log thus towing astern of the vessel, it is evident that at every revolution of the log-wheel *A B C D* an electric circuit between the telephone and the zinc and copper of the shank is made and broken by means of the copper strip *mm'* of the lignum-vitae roller coming into contact with the elastic tongue *R S*, each 'make and break' being signalled by a tick of the telephone. To

find the speed of the ship at any time, it is only necessary to count the number of ticks to the half-minute, as measured by the sand-glass, and read off from a converting table the number of knots and fractions corresponding to that number.

SAM HUBBARD.

Science for a livelihood.

I am interested in the communication from C. B., Brooklyn, N. Y., under the above caption in the issue of *Science* for Sept. 10. Like C. B., I graduated with a good scientific education, had done some practical work, and possessed a greater desire to labor in scientific fields than to do any thing else.

Instead of making application to only four schools, however, I applied to over sixty, and received a negative answer from all of them, and at the end of it was told by an eminent professor in Harvard university that there were at least fifteen applicants for every vacant place of the kind in the United States.

That was nine years ago, and my experience since confirms me in the belief, that if the student is without wealth, and has no friends who will forward him in his chosen field, he will do wisest, and be most independent, if he turns his attention to agricultural, mechanical, or any other honest occupation by which he can make some money; and then, after his money is his own, he can put as much of it as he sees fit into his scientific work. Such a course may be galling to pride, and a disappointment to friends, but, in all probability, there are few positions in this country where a student of small means can find sufficient work in the natural or experimental sciences to earn bread enough to keep the wolf from the door.

W. F. FLINT.

Winchester, N. H., Sept. 13.

Sea-water in the ears.

Science for Sept. 10 has a paragraph on this subject, but omits to mention that the momentum of tidal waves as they break upon the beach in this locality is sufficient to drive the water through the bather's nostrils, and up along the eustachian tube to the ears. In no other way is greater damage done to the ear in sea-bathing than this, since one cannot voluntarily close both mouth and nose, like marine animals, while bathing. I once saw a person go into the water with his nose embraced by a clothes-pin, and the greater number of bathers now protect the ears with wool. As I very well know from personal experience, it is not always easy to keep water out of the ears in surf-bathing, but I believe accidents from this cause are less frequent than formerly.

SAMUEL SEXTON.

New York, Sept. 11.

An easy method of measuring the time of mental processes.

Mr. Jastrow's method of measuring 'simple reaction time' by means of a circle of people, as described in *Science* of Sept. 10, was first used, as far as I know, by Dr. Holmes, who, as he said, "experimented with an apparatus more expensive than had ever before been used, and yet within the reach and means of everybody." The result obtained from this experiment depends largely on the experience of the operators, and it can easily be reduced to $\frac{1}{10}$ of a second.

If Mr. Jastrow will consult the *American journal of science* for September, 1871, he will find an account of some experiments on reaction and distinction time made by the writer, in which, in addition to color and form, the exercise of distinguishing tones of different pitch was introduced. The 'distinction times' given in that article are considerably greater than those obtained by Mr. Jastrow. The time of a single reaction only was measured, and I strongly suspect that in all cases the time obtained from measuring the duration of a series of reactions rapidly succeeding each other will be found to be shorter than that deduced from single measurements. The reason for this is obvious.

T. C. MENDENHALL.

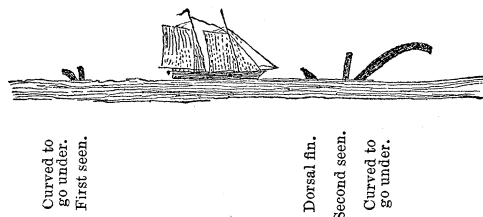
Washington, D. C., Sept. 13.

The sea-serpent.

With this please find an extract from an official report by Capt. Robert Platt, assistant coast and geodetic survey, with accompanying sketch of a 'sea-monster' seen by him near Cape Cod in October, 1878. Captain Platt is a trained observer, whose daily occupation at that time was to record just what he saw, and nothing more or less. I know Captain Platt so well that I have never doubted the existence of such a monster from the time his report was made known to me; and, if others have been sceptical, I hope that recent events have proven the matter beyond question.

[Extract from a report by Capt. Robert Platt, U. S. coast and geodetic survey, to the superintendent; written on board the U. S. coast-survey schooner Drift, Oct. 25, 1878.]

"I would also beg leave to state that Aug. 29, while becalmed off Race Point, Cape Cod, about four hundred yards from the vessel, we saw a sea-monster, or what I suppose has been called a sea-serpent. Its first appearance was that of a very large round spar two or three feet in diameter, from twelve to fifteen feet high, standing upright in the sea, but in a few minutes it made a curve and went down. It was visible about three minutes; the second appearance,



about half an hour after the first, the monster came out of the water about twenty-five feet, then extended to about thirty-five or forty feet, and about three feet in diameter; when out about forty feet, it curved and went down, and as it did so a sharp dorsal fin of about fifteen feet in length came up. This fin was connected to this monster, for the whole animal moved off with the same velocity. I looked at it with a good pair of glasses. I could not tell whether it had a mouth or eyes; it was of a brownish color. I enclose to you a rough sketch made by me, and submitted to all on board who saw the animal, and they all agree that it is a fair representation of the animal as it appeared."

B. A. COLONNA.

U. S. coast survey, Sept. 4.

SCIENCE.—SUPPLEMENT.

FRIDAY, SEPTEMBER 17, 1886.

PSYCHOPHYSICS.

THIS is a comparatively new science, although its beginnings can be traced back into the last century. But until comparatively recent years it occupied a subordinate position in speculative psychology, and the phenomena constituting its province were not assumed to be distinct enough for separate investigation. At present, however, a certain class of students are endeavoring, by experiment, to give its method and results that exactness which is supposed to describe the function of science proper. The province of science has become more exactly defined in the course of its development until the proper criterion of its function is that measurement and demonstration of its results which takes its theories out of the reach of probabilities and conjecture and establishes them upon a basis of certainty. Introspective psychology has either presented unsatisfactory results, or the universal prepossession for experimental effects has desired to represent it so, and thereby contrast its uncertainty with the tangible and demonstrable products of exact science. However we may account for it, psychophysics has come in to dispute the territory of the older psychology, at least in the person of some of its admirers. It likes to speak of purely introspective psychology as out of date, and as if it were discredited merely because it is of the past. Innovation and change have predisposed inquirers to enthusiasm for the new, perhaps because all the great triumphs of modern science have been conquests over old views, or deviations from them; the old has lost its prestige. Nothing has suffered more from this spirit than 'the old psychology,' as it is called by the admirers of psychophysics. The latter is taking rapid possession of scientific and philosophic interest, until students of the older philosophy are beginning to relax from their devotion, and to despair of retaining the homage which so many ages have paid to the idol of reflective thought.

Mr. Ribot's recent work on contemporary German psychology seeks to maintain and widen this breach between the two sciences: and we cannot but regret that it should be so; for they are really distinct sciences, running parallel with each other, and have no more reason to come into conflict with each other than physics and chemistry. Their methods may be different, but are not on that

account contradictory; and the one should not be made all-absorbing to the prejudice of the other.

We frankly admit, however, that it is no wonder the scientist, accustomed as he is to experiment and definite results, feels a sense of dissatisfaction with the study against which psychophysics presents the charge of obscurity. Kantian and post-Kantian psychology has never been characterized by perspicuity; and it is a natural revolt against it that even speculative Germany seems to have abandoned the popular gods of philosophy to find a new worship in experiments and facts quite in contrast with the genius of that people, disposed in so many particulars to take the high *a priori* road to truth, and to project every thing from consciousness, as it is accused of doing. Hence there is something of justice in the claim of psychophysics: it does tend to make its conclusions intelligible to experience; and that is a very great gain. But, with these legitimate claims to our respect, it should not usurp the whole province of psychological experience, which it does not do, nor repudiate introspection as a proper source and method of knowledge, which it is too much disposed to do, forgetful of the fact that in so doing it really undermines the final test of its own results.

The field of psychophysics is much more limited than one would at first suspect. Its name might imply at least a partial combination of physiology and psychology: but its advocates exclude the main and distinctive features of both these sciences from it, and assign it a very limited territory; as Dr. Wundt affirms, the field 'between inner and outer experience.' This means that it confines its investigations to phenomena which intermediate between purely mechanical events and purely reflective consciousness. Hence, on the one hand, such phenomena as circulation, assimilation, digestion, and on the other, such as perception, judgment, reasoning, memory, and imagination, are excluded from the field of its inquiries. Thus it is limited to the phenomena of sensation, which constitute the intermediate class spoken of. But even this class is not considered in its qualitative, but only its quantitative relations, hence it is still more limited. These quantitative characteristics consist of their intensity, psychic constancy, and reaction time. The last may be included under that of psychic constants, making two distinct problems for psychophysical investigation. That of the psychic constants is the more important of the two, as it has a bearing upon the speculative

problems of psychology : it is concerned mostly with the measurements of time and space, or with those primitive experiences which determine the genesis of our empirical conceptions of them.

The quality of sensation is not a subject of experiment, but the characteristic of intensity gives rise to what is known as Weber's law, which designs to express the relation between stimulus and sensation in respect of their quantity. It is found that sensation does not increase in a direct ratio with the increase of stimulus : and hence the law is formulated to express a geometric ratio in the increase of stimulus, and an arithmetic ratio in the increased intensity of sensation ; or, inasmuch as the absolute increase of stimulus is not always the same to increase sensation, it has been expressed to indicate that "sensation grows with *relatively* equal increments ;" that is, the *ratio* between the quantities of stimulus is always the same, whatever the absolute quantities may be. This law is quite accurate within a certain range, but requires modification as we approach the maximum and the minimum of sensation. The ratio between stimuli is not the same for different forms of sensation, but varies within a large degree, although it still sustains its geometric relation. This fact, as well as something of the scientific accuracy claimed for the science, will be evident in the following table of measurements, giving the ratio between stimuli required for the several senses. The numbers indicate that any given stimulus must be increased by the amount of itself expressed by the fraction in order to produce a perceptible change in sensation. Nothing has been determined for taste and smell.

For touch.....	1-3
For muscular effort.....	1-17
For temperature.....	1-3
For sound.....	1-3
For light.....	1-100

Thus any given object or resistance must be increased by one-third of its force in order to produce a perceptible increase of sensation ; and so on with the remaining senses. But the question arises, How far do such results give mathematical accuracy and exactness to the science of psychophysics ? It is claimed that its accessibility to experiment gives it the proper exactness of a science, and that the old psychology is a mere jumble of verbal disputes. But the admirers of psychophysics forget both their own admissions and the ultimate court of appeal for their conclusions, as well as the nature of the phenomena to be measured.

We have only to consult the above table to discover that only the stimuli are expressed in dis-

tinct quantitative relations. It is true that these can be definitely measured, because they are objective quantities like all other commensurable forces. But it is very different with the intensity of sensation, although Weber and Fechner presumed to express its increase in an arithmetic ratio, with the geometric ratio of stimulus. As a matter of fact, the sensation and its increase are not measured in terms of the exciting cause : if they were, something of scientific accuracy would be given the results. But as it is, the only distinct knowledge we have when there is a definite increase of excitation, is, that there is a perceptible change in the intensity of sensation. All mathematical formulae to express one sensation in quantitative relation to another are purely gratuitous : whether one sensation is once, twice, or three times as intense as another, no one can presume to declare with mathematical definiteness, because there is only a subjective criterion for intensity of sensation, and such a criterion affords no commensurating unit for others. This is admitted by psychophysicists themselves in complete unconsciousness of its significance against the claims of mathematical and scientific accuracy for psychology.

"Doubtless," says Mr. Ribot, an enthusiastic defender of the new science, "our states of consciousness are undetermined magnitudes. But is it impossible to determine them, that is, to submit them to measure ? The essential condition of measure is, that there be a fixed relation between the measure and that which is measured ;" and he elsewhere observes that "there is no unit or common measure to which we can refer two sensations to determine their intensive magnitudes." A still more forcible statement and admission is the following :—

"We assert without hesitation that it is brighter at mid-day than by moonlight ; that the firing of a cannon makes more noise than the firing of a pistol. There is, then, a quantitative comparison of sensations ; but we can only say there is equality or inequality, never *how many times* one sensation is greater than another. Has the sun a hundred or a thousand times more brilliancy than the moon ? Does a cannon make a hundred or a thousand times more noise than a pistol ? It is impossible to answer this question. The natural measure of sensation that each man possesses reveals to him the more, the less, the equal, never the *quantum*. Our determinations are always vague and approximate."

Such admissions should certainly modify the pretensions of psychophysics to an accuracy not claimed or possessed by the old psychology, and they do prove fatal to the claim of any such ex-

actness as is enjoyed by the physical sciences. The truth and importance of the law within the range of stimuli will not be denied; but as long as it is a mere assumption for purposes of definite expression that intensities of sensation are mathematical multiples of each other, there will be no reason for supposing one ratio rather than another, and hence the law proves absolutely useless for determining any exactness in psychology. In any case it could reach it only by the introspective method, which psychophysicists are so disposed to disparage, and yet only a little reflection is required to observe that introspection in one of its phases is the only valid testimony to the results already obtained and formulated. The truth is, there is no intelligible reason for setting up an opposition between introspection and experiment in order to place psychophysics in the rank of exact sciences. The question should not be as to its exactness or mathematical expression, but it should be the truth of its facts and conclusions. Conformity with mathematical laws and expression is not the sole criterion of truth or science, and hence by insinuating it the investigator but declares the transient and ephemeral nature of his speculations.

The problems and phenomena of psychic constants are much more interesting and important. They are welcome also as giving much more definiteness and intelligibility to some of the questions of transcendentalism, and, far from contradicting it, they seem to confirm it. The illusions producible in our conceptions of time and space under various circumstances stimulated inquirers to experiment for some constant in our various space determinations, and to measure the duration of psychic phenomena, or the intervals between stimulus and sensation, in order to find some constant for time. The time was when these two data of intelligence were supposed to be fixed and invariable, but further observations show them exposed to all the illusions belonging to perception in general, and hence the question arose both as to their origin and their nature. Transcendental philosophy anticipated experiment in making them ideal, but it was more successful in talking about them than it was in making its views clear and intelligible. Experimental psychology has come in to furnish us with definite data for reconsidering our empirical conceptions of them.

The nervous organization exhibits very different susceptibilities in different parts of the body: in some portions of the sensorium distinct and co-existent sensations are more nearly related in space than at others. In some cases it is also difficult to distinguish direction in the moving cause

of sensation. Thus in different stages of maturity and development, space relations vary in definiteness. The sensibility of different parts of the body has been accurately measured and tabulated, so as to show the different ranges of experience in sensation. For the palm of the hand, the finger-tips, the back of the hand, the arms, the shoulder-blade, the back, different parts of the face, the soles of the feet, etc., sensibility varies, both in respect to the threshold of sensation and the determination of separate excitations. In some cases there is only consciousness of affection, and no distinct knowledge of location. And in the case of vision the illusions respecting geometrical dimensions are indefinitely numerous; so that serious doubt may be raised as to the correctness of our ordinary spacial judgments, and some other constant must be demanded for theoretical purposes than is found in practical experience.

Hence the problem has been to find whether vision or muscular effort was the more accurate in the determination of space. But experiment has succeeded only in showing the corrective influence of one sense upon another without discovering any fixed conception to serve as an invariable measure for space. And so with the experience of time. Innumerable experiments show that our conception of duration can be varied with all sorts of circumstances: sometimes it appears indefinitely long, and at others incredibly short, while other facts go to prove that there was no difference in the two instances. Now moments may seem an age, and again there seems no interval of time between the beginning and the end of hours. And again our conception of time is influenced by the period required to realize an event or experience: it may be lengthened or shortened by the state of vitality, or the state of attention and application. The time between stimulus and sensation is different in different individuals, and yet it cannot be determined by subjective measurement; so that some other constant must be assumed to prove variability in any case. Hence there is a resort to heart-beats, or to certain forms of rhythm, as the better representatives of our definite conceptions for time, and perhaps to certain forms of co-existence as criteria for definable space. Thus space and time do not appear as absolute and simple as supposed in the older philosophy, but relative and complex, at least in experience. Other mental phenomena must be considered in our notion of them.

The attainment of such conclusions is due entirely to the experimental method, which has insisted upon actual demonstration of all speculations regarding ultimate conceptions. This sci-

tific spirit has added new interest to a study which was threatened with neglect, because it was too content with mere assertion, and presumed upon the self-evidence of words to communicate its wisdom. Although it may determine nothing as to the quality of sensation and consciousness, it will do much to drive away the mist that has ever hovered over many psychological speculations.

Still experiments have not yet demonstrated the derivative nature of time and space, although they have gone far to make them a matter of intelligible consideration and discussion. They have shown the variability of our empirical conceptions of them, but have not destroyed their validity as postulates of experience, because no special sense-perception may be constant enough to supply a criterion of their fixity. Indefinite conceptions of them at least are always assumed. However we may seek for some regular and uniform experiences within the ken of consciousness to serve as constants for them, or as the phenomena which determine and represent our conceptions of them, we shall find by closer scrutiny that some notion of time and space is already postulated in the very phenomena supposed to give the psychic constants for them; that is, we shall in vain endeavor to go outside of time and space to discover events which will account for them, or present their genesis from non-spacial and non-temporal relations. But at the same time experiment is providing data to render them clearer and more tangible to ordinary reflection than older speculations. For space the theory of 'local signs,' both tactual and visual, is taking the place of transcendental conceptions; and for time, the theory of discontinuous states of consciousness that may be objectively regular and uniform in their causes.

Among the most important contributions, however, which psychophysics has given to science, are the results showing the differential functions of the nervous system. The sense of temperature has been shown to be as distinct from touch as that is from vision, and even a different nerve is required to perceive cold from that which perceives heat. How far this differentiation of the sensorium may be carried, no one can predict. But even the established conclusions of the present will exert a far-reaching influence upon psychological speculations, and none more than the fact that distinct nervous organisms are required to receive representations once supposed to be connected with the same sense. It is too soon to predict what influence it will have in modifying older views: it will certainly modify them, but there is always a truth, even in the past, that avails to survive the mortality of language; and, although psychophysics may compel us to reconstruct some theories, it will not wholly do away

with the intellectual conquests of history, or oblige us to cast dust in the face of introspective methods, merely to gratify and strengthen an unnecessary prejudice against older opinions.

J. H. HYSLOP.

ANATOMICAL AND MEDICAL KNOWLEDGE OF ANCIENT EGYPT.

In a paper read at a recent meeting of the Royal institution of Great Britain, Prof. A. Macalister gave an account of the ancient anatomical and medical knowledge of Egypt, of which the following is a summary from the *Lancet*.

The surviving fragments of the early literature of Egypt are mainly of a religious character; but this is not to be wondered at, for the genius of the people was essentially religious, and their doctrine of the future state leavened their national life in almost every particular. To them the body was an integral part of the immortal humanity: therefore it could not be permitted to turn to decay, but had to be preserved from corruption that it might be a fit receptacle for the soul to dwell in through eternity. Their treatment of the body was thus dependent on their belief of its relation to the soul, and this, we learn from their religious writings, was a relationship of eternal independence. To secure perpetual preservation, the body had to be properly embalmed, the cavities opened and subjected to the action of antiseptics. Although the body was sacred, under the special protection of the god Thoth, though each part was under the guardianship of a special divinity, yet this sacredness did not preclude careful inspection and the processes necessary for preservation, for all parts had to be perpetuated.

Embalming was a religious rite, to be performed by the priests of the Cultus; and the historian Herodotus has preserved for us what is doubtless a substantially accurate account of the different methods whereby it was done in the later times in which he lived. The organs removed from the bodies of persons of the better classes were not returned into the body, but were preserved in vases of alabaster or stone, surmounted by the heads of the four divinities of Hades, the sons of Horus and Isis.

During the ascendancy of Greek influence in Egypt, Alexandria earned the reputation of being the chief school of anatomy and medicine in the world. Erasistratus, who lived in the days of Ptolemy Soter, B.C. 285, was an anatomist of such enthusiasm, that he and his disciples received from the king criminals condemned to death.

But this Alexandrian school, although upon Egyptian soil, was essentially Greek in spirit: even Herophilus had learned some of his anatomy

from Praxagoras of Cos, although, as the anatomy of the earlier Greek school had been derived from Egypt, it was but returning to the mother-country the traditions of culture derived therefrom. It was in Egypt Democritus of Abdera studied, and so was fitted to teach anatomy to Hippocrates, the father of medicine. The three pithy and graphic letters on anatomy (which are extant), which it is supposed Democritus sent to Hippocrates, may well have been the result of his Egyptian training. At a later period it was at Alexandria that Galen pursued his study of anatomy under Heraclianus, and the anatomical school of Alexandria survived until the Mohammedan invasion of Amru in A.D. 640.

That much even of the earlier Greek medicine, anatomy, and pathology was derived from Egypt, we learn both directly and indirectly. Most of the vegetable drugs in use in Greece were natives of Egypt; and Galen, speaking of one prescription called 'epigonos,' tells us that it was obtained from the adytum of the temple of Ptah, at Memphis. He quotes it, and other Egyptian prescriptions, from the book *Narthex*, written by Hera of Kappadokia.

Medical colleges of far greater antiquity than that of Alexandria existed in the priestly schools of Memphis, Heliopolis, Sais, and Thebes. These were much more faithful exponents of the purely Egyptian system of the art of physic.

Of the ancient medical literature of Egypt, two nearly complete treatises are still extant, and six or seven fragments of others. These vary in date and in perfection. The most complete are the Papyrus Ebers and the Medical papyrus of Berlin. The fragments which are noteworthy are, the British museum papyrus, formerly the property of the Royal institution, the Papyrus VI. of Boulaq, the Magical papyri of Turin and Paris, the Coptic medical manuscript in the Borgia library, and the Greek papyri 383 and 384 of Leyden.

ECONOMIC STATISTICS.

THIS volume is another of the *handbuchs* which the Germans of this generation are diligent in compiling. Encyclopedic in character, it deals with the statistics of production and consumption of economic goods in all countries where numerical data can be obtained. The germ of the work is discovered in a volume published by the same author in 1867, which presented certain commercial statistics collected during an extensive voyage around the earth. Since then two similar world-

tours have been undertaken, while official reports have been ransacked to yield up their treasures. The result is this book, containing a mass of statistical information in regard to almost every conceivable commodity which nourishes man, or which enters into manufactures as raw material. The scope of the work embraces the statistics of minerals, mechanical forces, machinery, steam-power, electricity, money (both paper and metal), waterways, railroads, postal service, telegraphs, marine cables, and telephones.

To illustrate the plan pursued, the section treating of grains is here analyzed. At the outset is given the proportion of the area of Europe which is devoted to the culture of the several cereals during successive decades in different countries. The condensed tables reveal the wheat situation at once. Unfortunately, however, in such a work as this the picture cannot be a late one, and is useful in large part only for purposes of comparison.

No figures are apparently given for any year since 1883; but the author, in arriving at averages, has been careful to choose periods of legitimate length to be used in such comparison. The tables are re-enforced not only by summaries of the crops produced, the exports and imports, and the consumption, both total and per capita, but also by historical and descriptive matter. One table shows us the source of English wheat in successive years. Five pages are set apart to the statistics of the United States, while the South American grains receive their share of attention. This is finally all summarized in one short tabulation, presenting the total world-production of wheat, rye, barley, oats, and corn, in hectolitres and centimetres.

There is little attempt to generalize from these census wastes of figures; but no one can pick his way through this interlacing of exports and imports, as here interwoven, without reflecting upon the immense fact of the internationalism of trade. But a century ago the total of the world's export trade in corn was but eleven million hectolitres, while now it annually reaches five hundred and fifty millions.

The fortunes of some comparatively insignificant commodities are tracked from country to country. Neither pork-packing in Chicago nor the trade in human hair is neglected; and no fact is too minute for this fact-hunter, who carefully informs us that the skilful hair-trader distinguishes between French and German hair by the sense of smell. One of the most valuable tabulations is that on p. 640, summarizing the present condition of the world's trade. Naturally the grand total values of imports and exports do not exactly balance, the former being about ten per cent in excess

Das wirthschaftliche leben der völker. Ein handbuch über production und consum. VON DR. KARL VON SCHERZER. Leipzig, Dürr, 1885. 8°.

of the latter: these are 35,691, and 32,645 million marks respectively. Of this amount, our own country, fourth in the race, furnishes about one-tenth; Great Britain leads with one-fifth; while France and Germany hold the intermediate positions. By such tables the work supplements and often corrects Mulhall's statistical volumes, and is more satisfactory in so far as Dr. Scherzer is more generous in stating his authority for statistics, which are necessarily more or less a matter of dispute. This literary accomplishment is one not yet acquired by Mr. Mulhall. Especially desirable are such references when the statistics of gold and silver are given. Not a little of the confusion of the present discussion concerning the merits of bimetallism is due to the conflicting statistics of gold and silver production; and all writers on the subject should be careful to state their authority when using such figures as a basis for argument. Here Dr. Scherzer follows Neuman-Spallart and Soetbeer. The work is scholarly and painstaking, and will be of service to all students desirous of new statistical conclusions or verification of others' work.

CONN'S EVOLUTION OF TO-DAY.

THIS book is defined by the author as "a summary of the theory of evolution as held by scientists at the present time, and an account of the progress made by the discussions and investigations of a quarter of a century." The book, however, deals chiefly with the evolution of animals. Inorganic evolution is dismissed with some few words about the nebular hypothesis, and, partly in statement and partly by implication, the author expresses the view that inorganic evolution is scarcely worthy of treatment by scientific methods and by scientific men. In so doing, he ignores the entire field of geology. In a manner equally curt, the subject of vegetal evolution is passed over, and the author begins his theme proper, which is a discussion of the nature of the evidence for and against the doctrines of animal evolution. He nowhere gives a clear and comprehensive definition of evolution, though the introduction is largely devoted to a discussion of the term, and to a denial that evolution is equivalent to Darwinism. Throughout the book an evolution of animal forms is maintained, but the doctrines taught by Darwin, as understood by the author, are, in general, though rather vaguely, denied. The reader is made to feel, that, in the author's mind, Darwinism is the name of something wicked that good people must disavow; and,

while the author reaches the conclusion that evolution is probably true, he wishes it to be understood that there is no taint of Darwinism in his beliefs.

The first chapter treats of the mutability of species, in which various facts, arguments, and opinions, *pro* and *con*, are briefly set forth, and an attempt made to derive an average therefrom; as if a mean result of contradictions could be used as a proximate truth, in the same manner that a mean of instrumental observations is used as an approximate determination. The same error, but in a minor degree, lurks in the remaining chapters.

In the second, third, fourth, and fifth chapters, the author reviews the arguments for evolution derived from the principles of classification, the paleontologic succession of forms, the development of the embryo, and the geographic distribution of animals. In these four chapters he skilfully and fairly characterizes four lines of inductive reasoning by which the specialization of a multiplicity of forms is demonstrated, and also, though not quite so clearly, shows how progress towards higher forms results therefrom. This part of the book, which is the body of the work, has great merit as a popular and fair discussion of the subject of the evolution of animals. It is reasonably devoid of technical terms, while broad facts and general principles are happily stated and explained to the understanding of intelligent readers who are themselves not specialists in zoölogy. In this respect the book is timely; and the general reader can gather therefrom a very good conception of the doctrines of animal evolution, and the status of development-opinions among scientific men, and of the new problems connected therewith that are arising through expanding research. The author has made as successful an exposition of this subject as, perhaps, is possible by this method of treatment, which is a characterization of facts and arguments, in lieu of a grand marshalling of the facts themselves,—it being the plan of the author to write for the general public rather than for the smaller body of scientific men.

If the reader of Mr. Conn's book could have a preliminary study of some one order of plants or animals, or of some line of embryologic development, or if he could study the origin and structure of some mountain-range, or the geology of some river drainage-system, so as to be able to fully appreciate the multitudinous facts that are gathered into some simple induction by the patient labors of modern scientific research, the general characterizations of the author would have a profound effect. Perhaps no man may have a very clear comprehension of what the

doctrine of evolution is, until he has had an objective study in at least some narrow field of research.

The new questions and accessory principles which are rapidly springing up about the central doctrines of evolution are pretty well set forth in the seventh chapter, entitled 'More recent attempts to explain evolution.'

The last chapter is on the evolution of man, and it is altogether unsatisfactory. It seems to have been written as a logical complement to a work on animal evolution, but it deals rather more with moral and metaphysical speculations than with the facts of the science. So far as it treats of human evolution, aside from its speculations, it refers simply to the animal man in his zoölogic relations. Human evolution, that is, the development of those characteristics which make man *man*, — the growth of human activities, — is ignored, and yet this is the largest subject in the literature of the world, embracing, as it does, the evolution of arts, the origin and development of institutions, languages, philosophies, or opinions, and all modern scientific psychology.

But a very small part of human evolution is embraced in theories of man and monkey kinship. The origin and growth of the humanities, i.e., those things which characterize humanity, have always been the subject of history; and all history is now in process of reconstruction upon a sounder theory than any which has hitherto obtained, and every writer in his own field postulates evolution by discussing the origin and development of the art, the institution, the language, the philosophy, or the psychic operation of which he treats.

J. W. POWELL.

SIDGWICK'S HISTORY OF ETHICS.

THIS little book by Professor Sidgwick is a reprint of his article on ethics in the 'Encyclopaedia Britannica,' with considerable alterations and additions. As originally published in the encyclopaedia, it was necessarily quite condensed in style, and it still retains that character to a great extent, thus presenting a much greater quantity of matter than is usually found in books of the same size.

The work is designed especially for students, and it seems to us admirably adapted to its purpose. The compression of the style is perhaps a defect from a literary point of view, but this is of little consequence in a text-book. The work is divided into three parts, treating of Greco-Roman, Christian, and modern ethics respectively. It is evidently based, as the author himself says, on

Outlines of the history of ethics for English readers. By HENRY SIDGWICK. London, Macmillan, 1886. 12°.

a thorough study of the original authors, only certain small portions, chiefly in part ii., being written at second-hand. It is marked, too, by almost perfect impartiality, — a merit of the first order in an historical work, but at the same time one seldom found in so high a degree. The author has been engaged in controversy with many ethical writers, and it might have been thought that a history of ethics from his pen would partake of the same character. On the contrary, it is devoted almost exclusively to the work of exposition, with only occasional criticisms when they seemed really required to point out serious defects in the systems described.

In the first part, attention is mainly directed to the three great ethical philosophers of ancient Greece, — Socrates, Plato, and Aristotle; and, though less than fifty pages are devoted to them, their modes of thought, their leading doctrines, and their relations to each other, are very clearly brought out. The author also traces the connection between all the Greek ethical systems, and shows in an interesting way "how, from the spring of Socratic conversation, flowed the divergent streams of Greek ethical thought." The second part of the book is much shorter than either of the others, as it should be; for, whatever may have been the influence of Christianity on practical morality, it can hardly be said to have contributed much to ethical philosophy. In treating of modern ethics, Professor Sidgwick confines himself in the main to English philosophers, on the ground that his work is intended for English readers, and that English ethical thought has developed itself, for the most part, independently of foreign influence; to which he might have added, that English ethical philosophy is by far the most important that has appeared in the world in modern times. The doctrines of the various English philosophers are briefly but clearly outlined, and special care is taken to point out the positive contributions of each thinker to the ethical thought of the world. Professor Sidgwick's book can be heartily commended to all who wish for information on the important and fascinating subject of which it treats.

THE PSYCHOLOGY OF REASONING.

M. BINET, a prominent member of the Society of physiological psychology in Paris, has been busy for many years in experimenting upon hypnotic subjects, who seem to be so abundant and interesting in France. He has formed one of a small band of workers, with Charcot as their

La psychologie du raisonnement, recherches expérimentales par l'hypnotisme. Par ALFRED BINET. Paris, Baillière, 1886. 12°.

head, who have brought to light many striking and remarkable facts about these abnormal conditions. This work suggested to M. Binet that some light might be shed on the nature of the reasoning process by observing the half-conscious actions of hypnotics; and the book before us is the result of this suggestion. The volume has been called out upon a slight provocation, and its argument in brief is as follows. A perception may be compared to the reading of a book: we attend to the sense, and not to the letters; we read something into these black marks. So, too, our sensations are taken, not for what they are, but for what they stand for, for what they tell.

By means of these perceptions the mind forms images, which are its fundamental elements. It is these images that form our stock in trade, and their prevalent nature determines many of our peculiarities of mind. They constitute one's mental background, one's apperceptive bent. These images come into combination and suggest each other as well as fuse together. The laws that condition this process are the laws of association of ideas, on which the English psychologists lay such stress. A close analogy can be traced between a syllogism and the process of perception: the perception is the conclusion; it expresses a judgment; it says, for example, this is an orange. The remembered images which enable me to recognize this as an orange play the part of the major premise, for this too expresses the results of past experience; and the minor premise which is brought into relation with the major by a certain similarity is the sensation itself. The analogy is closer than this crude outline indicates, and is really a highly suggestive view of the matter. It makes the syllogism the fundamental process of the human mind. It makes the triad, in which a middle term acts as the go-between for two others, of the utmost importance. This is the mechanism of reasoning, the general formula for getting valid deductions, as well as a fundamental natural process of the human mind. Man is thus in a new sense a rational animal; reasoning is a sort of new sense.

In the course of the development of this argument many interesting and valuable facts are brought out. It is only just to the author to notice a few of these. One result of his experimentation on hypnotics, and one physiological point, will serve as samples.

The subject, an hysterical young girl, was told that M. Féré (an associate of M. Binet) would be invisible to her. From that moment on, she ran against him, and thought it a miracle that she should be opposed by something she could not see: a hat on his head seemed suspended mysteri-

ously in the air. At the close of the session they forgot to disabuse her of this forced idea, and three days later M. Féré was still invisible; and, what was more remarkable, it was found that she had lost all remembrance of him: she knew neither his name nor his person, although he had been her friend for ten years. When he was made visible, she did not recognize him. At this period she had an hystero-epileptic attack, and from then on, M. Féré was her old friend as before. This case is used to illustrate the law of regression, which requires the most unstable and latest acquired knowledge to go first in dissolution, and to be re-acquired last in evolution. The patient, in recovering, first recognized M. Féré as an object, then generally as a man, and lastly particularly as her old friend.

In discussing the topic of the criterion of the difference of two sensations, the point is made that two sensations are distinct when they have a different local sign, a differently arranged group of accessory, secondary sensations. Two compass-points are felt as two when they have sufficiently different local signs. This local sign means that they can be localized. M. Binet tries the experiment, and finds that when two points are at such a distance apart as always to seem distinct when simultaneously touched, then, when either is touched separately, one can decide with confidence whether the touched spot is to the right or to the left, i.e., one can localize the sensation; but when the distance between the compass-points is less than this, the points are localized correctly only half the time, i.e., as often as the action of mere guessing would bring about. This point is a really valuable contribution to the psychology of touch. M. Binet's study can be recommended for its suggestiveness and the facts incidentally noticed, as well as for his ingenious analogies between psychology and logic. J. J.

THE *Medical and surgical reporter* gives the following interesting facts concerning the water-supply of the European capitals: Rome heads the list with her 204,000,000 litres of pure water every twenty-four hours (her population being 345,036, every inhabitant can dispose of 591 litres per diem); London comes next, for every one of whose 4,085,040 inhabitants there are 300 litres daily; Paris takes the third place, her population amounting to 2,240,124, and each inhabitant having for alimentary uses 58 litres per diem, and for secondary purposes 169, — a total of 227 litres; Berlin has 1,302,283 inhabitants, with 140 litres daily to each; Vienna, 770,172, 100 litres each; Naples, 463,172, with 200 litres; and Turin, 278,598, with 98 litres a head every twenty-four hours.